

between Jukussina and Yonesawa, I first found large bamboo plantations near the last place, about 1000 feet above sea-level, and $37^{\circ} 55' N$. Between here and Niigata the temperature of the coldest month must differ by about 3° , the latter place being situated near the sea. This gives about $30^{\circ} F$. for Yonesawa, or about the same as at Yusawa. Now in Great Britain, the mountainous districts excepted, the mean temperature of the coldest month is nowhere lower than 36° . A. WOELKOF
St. Petersburg, December 19, 1880

IN my letter (vol. xxiii, p. 194) I inadvertently stated that Sequoia cones were composed of from 16 to 20 scales. I intended to say 16 to 50, which appears to be the maximum number in either of the existing species. J. S. G.

Chalk

THE objections urged by Mr. S. N. Carvalho, jun. (vol. xxiii, p. 194), to Wallace's explanation of the deposition of chalk must have occurred to every geological reader of "Island Life." There are very many other objections to it, and I trust to be permitted to call attention to them in the *Geological Magazine*, as they are probably too purely geological to interest the readers of NATURE. J. S. G.

Average Height of Barometer in London

IT was stated in your "Meteorological Notes" a week or two ago in regard to the paper by Mr. H. S. Eaton on the average height of the barometer in London, that "the series is sufficiently extended as to entitle it to be considered one of the most valuable we possess in dealing with questions of secular meteorological variation."

Regarding it in the same light I have thought it worth while to apply Mr. Meldrum's method for discovering the existence and character of the secular variation in the sun-spot cycle. Taking the period 1811-79 I find the following figures for the mean cycles:—

LONDON

Annual Barometric Abnormals, Mean Cycles

Maximum years in fifth line.		Maximum years in seventh line.	
Pressure (1811-77)	Sun-spots (1811-77).	Pressure (1816-79).	Sun-spots (1816-79).
1. +0'006 ...	-33'9 ...	-0'005 ...	+23'3
2. + '016 ...	-23'4 ...	- '001 ...	+14'5
3. + '013 ...	0'0 ...	- '001 ...	+4'8
4. - '002 ...	+28'2 ...	- '003 ...	-5'6
5. - '010 ...	+43'1 ...	- '005 ...	-19'0
6. - '011 ...	+34'2 ...	- '001 ...	-32'5
7. - '007 ...	+16'8 ...	± '000 ...	-37'1
8. + '001 ...	+0'2 ...	+ '011 ...	-25'4
9. ± '000 ...	-14'2 ...	+ '021 ...	+1'8
10. + '001 ...	-24'2 ...	+ '010 ...	+30'9
11. ± '000 ...	-26'3 ...	- '003 ...	+44'8

The variation of pressure, though not so regular as that I worked out for St. Petersburg in 1879, is of an almost exactly opposite character, the minimum pressure appearing as in India, about the time of maximum sun-spot, and the maximum pressure lagging two years behind the epoch of minimum sun-spot. These results agree with the known annual rainfall variation in the same cycle, which is likewise similar in character to that which occurs in the tropics. I would suggest that the marked difference between the results for London and St. Petersburg possibly arises from the close communication between England and the tropics through the medium of Atlantic oceanic and atmospheric currents. E. DOUGLAS ARCHIBALD

January 4

Experiments with Vacuum Tubes

IN my letter published in the last number of NATURE I omitted to say that we have compared vacuum tubes without electrodes

with a tube containing water. A tube was filled about nine-tenths full of water and then sealed hermetically. It was then applied to the prime conductor of the electric machine and electrified in the same way as the vacuum-tubes without electrodes, and it was found to behave precisely as they did. The water tube became charged as a double Leyden jar, positive outside and negative inside at the end next the prime conductor, and negative outside and positive inside at the other end. A great tendency to rupture of the glass was also observed. So far as we have been able to see the most perfect vacuum that I have been able to obtain with the Sprengel pump has behaved as to frictional electricity precisely as a perfect conductor such as water.

These experiments seem interesting in connection with the discoveries of Mr. Crookes as to the properties of a very perfect vacuum. No doubt it was known that flashes can be obtained within vacuum tubes without electrodes; but the properties of a perfect vacuum as a conductor of electricity has not been hitherto sufficiently investigated. J. T. BOTTOMLEY

Physical Laboratory, the University, Glasgow, January 8

Oxidation of Quinine, &c.

IN the Chemical Society's *Journal* for December, 1880, there is an abstract of a paper by Hoogewerf and Van Dorp, published in *Liebig's Annalen*, cciv. 84-118, in which the authors describe experiments on the oxidation of quinine, quinidine, cinchonine, and cinchonidine. As reference is made in this paper to our work upon the same subject in such a manner as to lead to the inference that we had copied Hoogewerf and Van Dorp, we beg to call attention to the dates of publication of the various memoirs relating to the matter.

In the Berlin *Berichte*, x. 1936 (close of 1877), Hoogewerf and Van Dorp published a preliminary note on the oxidation of aniline, toluidine, and quinine, and stated that they had obtained amongst other products of oxidation of quinine a nitrogenous acid, to which apparently they attached little importance. Of this acid they gave no further account. At that time we were working at the same subject, and had come to some important conclusions.

As Hoogewerf and Van Dorp's results contained nothing relating to quinine in addition to what had been observed by Cloez and Guignet many years previously, we did not consider that they were entitled to claim that this field of work should be reserved for them. We therefore sent our paper to the Chemical Society, before which it was read on January 19, 1878 (see also Berlin *Berichte*, xi. 324). In this paper we stated that the acid obtained by us from quinine was probably identical with dicarbopyridenic acid. That the acid was a pyridenic acid we had no doubt, but owing to the difficulty of purification we had not been able to establish its formula with certainty.

In the Berlin *Berichte*, xii. 158-161, was published a second paper by Hoogewerf and Van Dorp (read before the Berlin Chemical Society on January 27, 1879), on the acid obtained from quinine, giving no analyses, but stating that the acid was *tri-* and not *dicarbopyridenic* acid, thus confirming our result in its important bearing, viz. the connection between the quinine and pyridine series. In the same paper they suggested that an acid obtained by them from quinidine and cinchonine was identical with the quinine acid.

Immediately on receipt of the number of the Berlin *Berichte* containing Hoogewerf and Van Dorp's paper, we forwarded to the secretary of the Chemical Society our second memoir, which contained numerous analyses of the acid obtained, not from quinine only, but also from the allied alkaloids, quinidine, cinchonine, and cinchonidine, together with a full description and analysis of all its important salts. That paper was read before the Society on February 20, 1879.

In *Liebig's Annalen*, cciv. 84-118 (July 31, 1880), or a year and a half after the publication of our second paper, Hoogewerf and Van Dorp published analyses of the acid and many of its salts, prepared from three alkaloids, the results confirming our own in all points.

Our claim, which the above dates fully substantiate, is to have been the first to establish the connection between the quinine and pyridine series, and to have proved that the four alkaloids all gave the same oxidation product.

Prof. Butlerow of St. Petersburg, immediately on appearance of our first paper, when engaging in work closely connected with, but not overlapping ours, wrote suggesting that we should

each confine himself to his own branch, at the same time recognising the importance of our discovery; and Herr König, in a paper published in the *Berichte*, xii. 97, referring to our first paper, says: "Es ist der erste glatte Uebergang der Chinaalkaloide in eine jedenfalls einfachere Substanz—das Pyridin."

WILLIAM RAMSAY
JAMES J. DOBBIE

Glasgow University

The Temperature of the Breath

DR. DUDGEON's first letter under this heading contained the suggestion of a friend that his enigmatical thermometric readings were to be accounted for by the high temperature "caused by the condensation of the moisture of the breath by the silk handkerchief." The discussion that followed has not only brought us back to this solution, but has also furnished us with an authoritative expression of opinion that the clinical thermometer is not sensitive to pressure. F. J. M. P. first hinted the contrary proposition only to have it thrust aside by Dr. Dudgeon with blunt denial, neglected by Dr. Roberts, and finally discarded by himself for no other apparent reason than that aqueous vapour in condensing liberates heat. Yet I venture to assert that readings as high as any obtainable by Dr. Dudgeon's method, less the pressure, can be obtained by a very similar mode of experimenting, without the developed heat: 1. If the bulb of a thermometer, protected by paper or other non-conductor, be squeezed in an intermittent manner between finger and thumb, it will be found that the mercury can readily be made to dance up and down through about a degree on the scale with a celerity not attributable to changes of temperature. 2. If eighteen inches of cotton thread be tightly wound about the bulb, on immersing the thermometer in water it will exaggerate the temperature sometimes by as many as 12° F. 3. If a tube filled with cacao butter be substituted for the thermometer the butter beneath the thread will be longer in melting than that in other portions of the tube, a result which I think proves that the high readings of experiment No. 2 are not temperature, but (in the light of No. 1) pressure readings.

My chief object in writing is to protest on general grounds against the treatment accorded to F. J. M. P.'s suggestion, but at the same time I wish to express my opinion that Dr. Roberts' argument would have been strengthened by giving heed to it, for I see nothing in *his account* of the interrupted experiment not explainable on the pressure hypothesis alone, the descending series of readings being perchance due to a yielding of the wrappings under prolonged tension. On the other hand I have to thank this omission on Dr. Roberts' part for having induced me to test the subject for myself, and thus experience, in repeating his experiment, the rare pleasure of scientific surprise at seeing the index mount higher and higher above the level of my expectations under conditions which left no doubt as to the cause being a rise of temperature. Dr. Dudgeon has done good service by directing attention to a simple experiment which, properly interpreted, throws new light on the philosophy of clothes, and should prove a telling shaft in the quiver of popular science.

WM. McLAURIN

Islington, December 26, 1880

IN the number of NATURE which reached Madras after the departure of the mail conveying my letter of the 9th inst., I was glad to read Dr. W. Roberts' abundantly full and lucid explanation of the heat produced by breathing on thermometers enveloped in hygroscopic substances. He has, by a very simple method, confirmed the view endorsed in my communication in NATURE, vol. xxiii. p. 534.

That the effects of friction and of compression of air are so slight that they may be disregarded, has been proved; and the rise has been clearly traced to absorption of aqueous vapour. It has yet to be determined how much of this heat may be accounted for by the reduction of aqueous vapour to the fluid state, and how much by capillary action and absorption of water, with or without chemical union, and its reduction to the solid state—all of which may be included in hygroscopic action. This determination would involve some intricate investigations which some scientific specialist may perhaps find leisure to undertake. That more than simple vapour condensation is concerned in the production of hygroscopic heat is shown by the rise of temperature on adding water to a non-saturated hygroscopic substance.

A scientific colleague has suggested to me that some cases of very high axillary temperatures may be explained by the clothing of patients being pressed into the axilla in contact with the thermometer. Thus, by folding a banian round a thermometer placed in the axilla, I registered a temperature above 100° F., while the temperature in the bare axilla was 98.3°. It is evident that recently changed and dried clothing and clothing warmed by the body of a non-perspiring fever patient would have still more effect when pressed closely into a hot and moist axilla. Although this point is important mainly to physicians, I venture to draw attention to it through your columns on account of its connection with the subject of hygroscopic heat.

C. J. McNALLY

Madras, December 16, 1880

Distance of Clouds

I HAVE conveniently determined the distance of passing clouds by a method probably not new, but which I have not seen described.

It consists in ascertaining the velocity with which the shadow of a cloud traverses level ground, which is easily observed, and of course gives the velocity of the cloud itself.

The angular motion per second of clouds passing overhead is simultaneously observed by means of a coarse micrometer in a telescope, or with a theodolite.

The distance is thus obtained with fair approximation.

Distance = $\frac{vt}{n}$, v being the velocity in feet per second, and n the number of minutes of arc described in t seconds.

A distant mirror may be advantageously used in determining the velocity of the shadow.

EDWIN CLARK

Fluke in Calves

CAN any of your readers account for the following facts?—An examination of the liver of some six-weeks-old calves which had never touched any food but their mother's milk showed them to be infested with fully-developed Fluke (*Distoma hepatica*). It is clear that the presence of these flukes does not admit of the usual explanation, viz., the ingestion with green food or water of mollusca bearing the larva in one of its earlier stages.

I should be grateful if any of your readers could suggest an explanation of the mode in which the fluke entered the liver of the calf. Is it possible that the larva may have passed into the milk of the mother, and so have entered the stomach of the calf?

It may interest some of your readers to know that traces of fluke were present in the livers of cattle lately killed when in high condition. The fluke had apparently been established in the liver some considerable time previous to the slaughter of the animals, and had perished on their attaining to a state of high health and vigour.

A. B.

JOHN STENHOUSE, LL.D., F.R.S.

IN the early morning of the last day of the old year we lost one of the few surviving founders of the Chemical Society, Dr. John Stenhouse. He was born at Glasgow, October 21, 1809, the son of William Stenhouse of the well-known firm of calico-printers, John Stenhouse and Co. of Barrhead. He was educated first at the Grammar School and then at the University of Glasgow, and long resided in his native city. At an early age he turned his attention to chemistry, and diligently studied that science under Graham and Thomson, and subsequently with Liebig at the University of Giessen. When he removed to London, after the failure of the Western Bank of Scotland had deprived him of the fortune bequeathed to him by his father, he became Lecturer on Chemistry in St. Bartholomew's Hospital, London, but was obliged to resign that appointment in 1857 owing to a severe attack of paralysis. Even this affliction however did not discourage him, and after the lapse of a short time he renewed his scientific labours. In 1865 he succeeded Dr.